February 23, 1952

Dear Frabcis:

Thanks for sending the mss. of your papers on lac- mutation. Esther is about to read them now; she also asks that I acknowledge the return of the microfilm. In the meantime, she has submitted a paper to Genetics, based on her thesis.

There were a couple of small points on your papers:

A)—JGM. p. 3 "ML known not to undergg recombination..."

p. 28 "pleimorphic" pleiotropic??

B) Nature Service de Physiologie (Microbienne ?)
p.1

In view of its possible pertinence to residual growth, you might be particularly interested in Englesberg & Stanier, J. Bact. 58:171, who discuss a very similar experience on the probable contamination of salts with carbon sources. Probably you had seen this previously.

In the Nature paper you cite recombination as a possible deviation from Lea & Coulson's postulates. How could this influence the mutant distribution? We were also interested in just what the K-12 strain was that you used— is it not one isolated directly from K-12 in your own lab?

Some time, we ought to have a convention to define a "genetic adaptation". I am not sure that <u>any</u> growth is restrictive enough. Would you call the penicillinase of B. cereus a genetic adaptation?— it would depend on your definition of the "intracellular factors responsible for the adaptation". One could well argue for so describing the penicillinase itself. My own preference for a genetic adaptation is one that is <u>potentially</u> capable of indefinite persistence when the adapted culture is propagated side by side with the original on an indifferent medium. This has its failings also.

I am not sure what your postcard meant, but am glad to enclose some ONPG. Dave Bonner has made some also. As you may imagine, our supply is a limiting factor when we don't have the facilities for an organic lab.

I understood you may be in attendance at the Iowa State Biostatistics Conference. I hope that you do plan to go, and that you will be able thereby to pay us a very welcome visit.

Sincerely,